



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

Postscript.

Just upon the close of the adjoynd Letter, I received from Mr. Gascoine, yours of May the fourth; wherein you are pleased to favour us with an exact account of the famous Experiment of the coloured Spectrum, lately exhibited before the Royal Society. I was much rejoiced to see the Trials of that Illustrious Company, agree so exactly with ours here, though in somewhat ours disagree from Mr. Newton, as you will understand by the inclosed impartial account from,

Sir, &c.

Mr. Newton's Answer to the precedent Letter, sent to the Publisher.

Sir,

THe things opposed by Mr. Line being upon Trials found true and granted me; I begin with the new question about the proportion of the length of the Image to its breadth. This I call a *new one*; for, though Mr. Line in his last Letter spake against so great a length as I assign, yet, as it seems to me, it was not to grant any transverse length shorter than that assigned by me, (for in his first Letter he absolutely denied that there would be any such length;) but to lay the greater emphasis upon his discourse whilst in defence of common Optiques he was disputing in general against a transverse Image: And therefore in my Answer I did not prescribe the just quantity of the refracting Angle with which I would have the Experiment repeated: which would have been a necessary circumstance, had the dispute been about the just proportion of the length to the breadth. Yet I added,* this Note, that the bigger the angle of the Prism is, the greater will be the length in proportion to the breadth; not imagining but that when he had found many Prism the length of the Image transverse to the axis he would easily thence conclude, that a Prism with a greater angle would make the Image longer, and consequently that by using an angle great enough he might bring it to equal or exceed the length assigned by me; as indeed he might: for by taking an Angle of 70 or 75 degrees, or a little greater.

* In my first Letter
in Phil. Trans. N.
1234 15099. 101

greater, he might have made the length not only five, but six or eight times the breadth and more. No wonder therefore, that Mr. *Lucas* found the Image shorter than I did, seeing he tried the Experiment with a less Angle.

The Angle indeed which I used was but about 63 degrees 12 minutes, and his is set down 60 degrees: the difference of which from mine, being but 3 degrees 12 minutes, is too little to reconcile us, but yet it will bring us considerably nearer together. And if his Angle was not exactly measured, but the round number of 60 degrees set down by guess or by a less accurate measure (as I suspect by the conjectural measure of the refraction of his Prism by the *ratio* of the sines 2 to 3, set down at the same time, instead of an Experimental one,) then might it be two or three degrees less than 60, if not still less: and all this, if it should be so, would take away the greatest part of the difference between us.

But however it be, I am well assured, my own observation was exact enough. For I have repeated it divers times since the receipt of Mr. *Lucas's* Letter, and that without any considerable difference of my Observations either from one another, or from what I wrote before. And that it might appear experimentally, how the increase of the Angle increases the length of the Image, and also that no body who has a mind to try the Experiment exactly, might be troubled to procure a Prism which has an angle just of the bigness assigned by me; I tried the Experiment with divers Angles, and have set down my Trials in the following Table; where the first column expresses the six Angles of two Prisms which I used, which were measured as exactly as I could by applying them to the angle of a Sector; and the second column expresses in inches the length of the Image made by each of those Angles; its breadth being two inches, its distance from the Prism 18 feet and four inches, and the breadth of the hole in the Window shut $\frac{1}{4}$ of an inch.

The Angles of		The Lengths of	
degr. min.		the Image.	
The first Prism	56 10	{	$7\frac{3}{4}$
	60 24		$9\frac{1}{2}$
	63 26		$10\frac{1}{3}$

The

<i>The Angles of</i> <i>degr. min.</i>		<i>The Lengths of</i> <i>the Image.</i>
<i>the second Prism.</i>	54 0	$7\frac{1}{3}$
	62 12	$10\frac{1}{8}$
	63 48	$10\frac{3}{4}$

You may perceive, that the length of the Images in respect of the angles that made them, are something greater in the second Prism than in the first; but that was because the glass, of which the second Prism was made, had the greater refractive power.

The days in which I made these Trials were pretty clear, but not so clear as I desired, and therefore afterwards meeting with a day as clear as I desired, I repeated the Experiment with the second Prism, and found the lengths of the Image made by its several angles to be about $\frac{1}{4}$ of an inch greater than before, the measures being those set down in this Table.

<i>The Angles of</i> <i>degr. min.</i>		<i>The Lengths of</i> <i>the Image.</i>
<i>the second Prism</i>	54 0	$7\frac{2}{3}$
	62 12	$10\frac{1}{2}$
	63 48	11

The reason of this difference I apprehend was, that in the clearest days the light of the white skies, which dilutes and renders invisible the faintest Colours at the ends of the Image, is a little diminished in a clear day, and so gives leave to the Colours to appear to a greater length; the Suns light at the same time becoming brisker, and so strengthening the Colours and making the faint ones at the two ends more conspicuous. For I have observed, that in days something cloudy, whilst the Prism has stood unmoved at the window, the Image would grow a little longer or a little shorter, accordingly as the Sun was more or less obscured by thin Clouds which passed over it; the Image being shortest when the Cloud was brightest and the Suns light faintest. Whence it is easie to apprehend, that, if the light of the Clouds could be quite taken away, so that the Sun

Sun might appear surrounded with darkness, or if the Sun's light were much stronger than it is, the colours would still appear to a greater length.

In all these Observations the breadth of the Image was just two inches. But observing, that the sides of the two Prisms, I used, were not exactly plain, but a little convex, (the convexity being about so much as that of a double Convex-glass of a sixteen or eighteen foot *Telescope*) I took a third Prism, whose sides were as much concave as those of the other were convex; and this made the breadth of the Image to be two inches and a third part of an inch; the angles of this Prism, and the lengths of the Image made by each of those Angles being those express'd in this Table.

<i>The Angles of the Prism.</i>	<i>The Lengths of the</i>
<i>degr.</i>	<i>Image in inches.</i>
58	$8\frac{1}{2}$
$59\frac{1}{2}$	9
$62\frac{1}{2}$	$10\frac{1}{4}$

In this case you see, the concave figure of the sides of the Prism by making the rays diverge a little, causes the breadth of the Image to be greater in proportion to its length than it would be otherwise. And this I thought fit to give you notice of, that Mr. *Lucas* may examine, whether his Prism have not this fault. If a Prism may be had with sides exactly plain, it may do well to try the Experiment with that; but its better, if the sides be about so much convex as those of mine are, because the Image will thereby become much better defined. For this convexity of the sides does the same effect, as if you should use a Prism with sides exactly plain, and between it and the hole in the Window-shut, place an Object-glass of an 18 foot *Telescope*, to make the round Image of the Sun appear distinctly defined on the wall when the Prism is taken away, and consequently the long Image made by the Prism to be much more distinctly defined (especially at its streight sides) than it would be otherwise.

One thing more I shall add: That the utmost length of the Image from the faintest Red at one end to the faintest Blew at the

the other, must be measured. For in my first Letter about Colours, where I set down the length to be five times the breadth, I called that length the utmost length of the image ; and I measured the utmost length, because I account all that length to be caused by the immediate light of the Sun, seeing the Colours (as I noted above) become visible to the greatest length in the clearest days, that is, when the light of the Sun transcends most the light of the Clouds. Sometimes there will happen to shoot out from both ends of the Image a glaring light a good way beyond these colours, but this is not to be regarded, as not appertaining to the Image. If the measures be taken right, the whole length will exceed the length of the streight sides by about the breadth of the Image.

By these things set down thus circumstantially, I presume Mr. *Lucas* will be enabled to accord his tryals of the Experiment with mine ; so nearly, at least, that there shall not remain any very considerable difference between us. For, if some little difference should still remain, that need not trouble us any further, seeing there may be many various circumstances which may conduce to it ; such as are not only the different figures of prisms, but also the different refractive power of Glasses, the different diameters of the Sun at divers times of the year, and the little errors that may happen in measuring lines and angles, or in placing the prism at the window ; though, for my part, I took care to do these things as exactly as I could. However Mr. *Lucas* may make sure to find the Image as long or longer than I have set down, if he take a prism whose sides are not hollow ground, but plain, or (which is better) a very little convex, and whose refracting angle is as much greater than that I used, as that he has hitherto tryed it with, is less ; that is, whose angle is about 66 or 67 degrees, or (if he will) a little greater.

Concerning Mr. *Lucas*'s other Experiments, I am much obliged to him that he would take these things so far into consideration, and be at so much pains for examining them ; and I thank him so much the more, because he is the first that has sent me an experimental examination of them. By this I may presume he really desires to know what truth there is in these matters. But yet it will conduce to his more speedy and full
satisf-

satisfaction if he a little change the method which he has propounded, and instead of a multitude of things try only the *Experimentum Cancis*. For it is not number of Experiments, but weight to be regarded; and where one will do, what need many?

Had I thought more requisite, I could have added more: For before I wrote my first Letter to you about Colours, I had taken much pains in trying Experiments about them, and written a Treatise on that subject, wherein I had set down at large the principal of the Experiments I had tried; amongst which there happened to be the principal of those Experiments which Mr. *Lucas* has now sent me. And as for the Experiments set down in my first Letter to you, they were only such as I thought convenient to select out of that Treatise.

But suppose those had been my whole store, yet Mr. *Lucas* should not have grounded his discourse upon a supposition of my want of Experiments, till he had examined those few. For if any of those be demonstrative, they will need no assistants, nor leave room for further disputing about what they demonstrate.

The main thing he goes about to examine is, *the different refrangibility* of Light. And this I demonstrated by the *Experimentum Crucis*. Now if this demonstration be good, there needs no further examination of the thing; if not good, the fault of it is to be shewn: for the only way to examine a demonstrated proposition is, to examine the demonstration. Let that Experiment therefore be examined in the first place, and that which it proves be acknowledged, and then if Mr. *Lucas* want my assistance to unfold the difficulties which he fancies to be in the Experiments he has propounded, he shall freely have it; for then I suppose a few words may make them plain to him: whereas, should I be drawn from demonstrative Experiment to begin with those, it might create us both the trouble of a long dispute, and by the multitude of words, cloud rather than clear up the truth. For if it has already cost us so much trouble to agree upon the matter of fact in the first and plainest Experiment, and yet we are not fully agreed; what an endless trouble might it create us, if we should give our selves up to dispute upon every Argument that occurs, and what would become of Truth in such a tedious dispute?

The way therefore that I propound, being the shortest and clearest (not to say, the only proper way,) I question not but Mr. *Lucas* will be glad that I have recommended it, seeing he professes, that it is the knowledge of *truth* that he seeks after. And therefore at present I shall say nothing in answer to his Experimental discourse, but this in general; that it has proceeded partly from some misunderstanding of what he writes against, and partly from want of due caution in trying Experiments; and that amongst his Experiments there is one, which when duly tried, is, next to the *Experimentum Crucis*, the most conspicuous Experiment, I know, for proving the different refrangibility of Light, which he brings it to prove against.

By the *Post-script* of Mr. *Lucas*'s Letter, one not acquainted with what has passed, might think, that he quotes the Observation of the *R. Society* against me; whereas the relation of their Observation, which you sent to *Liege*, contained nothing at all about the just proportion of the Length of the Image to its Breadth according to the angle of the Prism, nor any thing more (so far as I can perceive by your last) than what was pertinent to the things then in dispute, *viz.* that they found them succeed as I had affirmed. And therefore since Mr. *Lucas* has found the same success, I suppose, that when he expressed, that *he much rejoiced to see the Trials of the R. Society agree so exactly with his*, he meant only so far as his agreed with mine.

And because I am again upon this first Experiment, I shall desire, that Mr. *Lucas* will repeat it with all the exactness and caution that may be, regard being had to the information about it, set down in this Letter; and then I desire to have the *length* and *breadth* of the Image with its *distance* from the Prism, set down exactly in feet and inches, and parts of an inch, that I may have an opportunity to consider what relation its length and breadth have to the Sun's diameter. For I know, that Mr. *Lucas*'s Observation cannot hold where the refracting angle of the Prism is full 60 degrees, and the day is clear, and the full length of the Colours is measured, and the breadth of the Image answers to the Sun's diameter: And seeing I am well assured of the truth and exactness of my own Observations, I shall be unwilling to be diverted by any other Experiments, from having a fair end made of this in the first place. *Sir, I am, &c.*

Post.

Postscript.

I Had like to have forgotten to advise, that the Experimentum Crucis, and such others as shall be made for knowing the nature of Colours, be made with Prisms which refract so much, as to make the length of the Image five times its breadth, and rather more than less; for, otherwise Experiments will not succeed so plainly with others as they have done with me.

An Account of two Books :

- I. *Traçtatus de VENTRIGULO & INTESTINIS*, cui præmittitur alius de *PARTIBUS CONTINENTIBUS* in genere, & in specie de *Partibus ABDOMINIS*; Auth. Franc. Gliffonio, M. D. & Coll. Med. Lond. Socio, nec non Soc. Regalis Collegiæ. Londini, 1676. in quarto.

THe eminently learned Author of this Anatomical Treatise, having presupposed the general Divisions of the parts of an Human Body, taken in their largest sense, and their inadequate conceptions, upon the account of which they are in divers respects called Similar or Organical, proceeds directly in this work to the Inferiour and more Practical divisions of the said Body.

And having first of all divided the Lowermost *Venter* into its Regions, and designed the parts contained in each of them; he goes on to the division of the *Cutaneous* parts, and considers the nature, structure, origin, vitality, and uses of the *Cuticula* and *Cutis vera*. Where we cannot but take notice, that the Author, as well here, as throughout this whole piece, builds much upon the grounds, he had laid in the Book, he published four years ago, *de Vita Naturæ*, wherein he ascribes much to *Natural Perception*, which he holds to be an Operation anterior to, and more general and more simple than that of, *Sense*; and in which Perception, accompanied with Appetition and Motion, he makes *Original Life* to consist, which, to him, is nothing else but the Energetical or Operative nature of any Being subsisting by it self, not producible by any external power, motion, texture, figure, organization or proportion of parts, but by the sole